

visited by bees, nor disturbed by being beaten by the wind against the surrounding net." The reviewer says:—"The *Field Naturalist* quotes the passage incorrectly, omitting 'when the flowers are neither visited by bees.'" In my chapter headed "The Sterilising Influence of Darwin's Net," where the quotation occurs, the bees in this reference—as they were excluded by the net—had nothing whatever to do with the subject, and so reference to them was omitted; the effect of the net and of the net alone on fertilisation was there being discussed.

Such are the passages which the reviewer cites as misquoted or interpolated. I should have esteemed it a deep dishonour if I had knowingly misquoted any statement of Darwin, or had interpolated any words in quotations from Darwin, and should not lightly have excused myself even had it been done carelessly or unwittingly. To avoid all such charges like those of the reviewer, I distinctly state in the preface:—"We have carefully given the references to all the passages quoted, or referred to, in the following pages." This was done that every reader might find without trouble, if he desired, the original passages and could compare the quotation with them.

At p. 409, the reviewer cites from "The Primrose and Darwinism":—"In calm weather the net would prevent the free access of the wind and would prevent it from shaking, and so from freely disturbing and distributing the pollen" (p. 8), and states "not a particle of evidence is given from his point of view." The evidence in this case is supplied by Darwin himself:—"In all cases the flowers were protected from the wind" (Cr. and S.F., p. 23); and again, as quoted in *Prim. and Dar.*, "The wind does hardly anything in the way of conveying pollen from plant to plant when insects are excluded" (F. of Fl., p. 93).

The reviewer says, "When the author ventures on suggesting a function we are liable to come across such a theory, as the orifice in the carina of *Lotus* is to serve for the ventilation of the pollen stored within the carina." As I spent three and a half to four years of my life in the uninterrupted study of physiology and its sister sciences, there still remains a sufficient residuum of its flavour in the cask that I can venture to assert that if your reviewer will only consult a competent physiologist about a pistil surrounded with packed pollen in a closed carina, like Fig. 13, p. 132 (Sowerby's "English Botany," v. iii.), of the *Lotus*, he will tell the reviewer that such ventilation of a cone, if not absolutely necessary in every season, yet would be absolutely necessary in some seasons, and would be very conducive in all seasons to the healthy fertilisation and fructification of the pod.

Finally, the reviewer states, "the author makes the astonishing statement that Darwin's predecessors are to be commended for strictly subordinating theory to natural facts. They thus happily avoided the error into which Darwin, in this instance at least, most assuredly and most conspicuously fell." The reference here is to the dimorphism of the primrose and to Darwin's statement in reference to such a state—"One form of *Primula* must unite with the other form in order to produce full fertility" ("Form of Flowers," pp. 49, 56). And again, "heterostyled flowers stand in the reciprocal relation of different sexes to each other" ("Form of Flowers," pp. 2, 28, 245).

The late Professor J. S. Henslow was acquainted with the heterostylism of the primrose as stated (and quoted) by me in the preface to the book, but Darwin alone fell into the error that "the two forms stood in the reciprocal relation of different sexes to each other." I will leave to the judgment of botanists who are also acquainted with the long-tongued *Hymenoptera aculeata* and *Lepidoptera* to decide the question in the spring by observing the flowers from the middle of March to the end of April, whether the short-styled primrose, though fully productive, is cross-fertilised by insects.

In the same way we will leave to all observers or naturalists, by their observing the flowers in the month of May, the question whether the *Arum* is not, with possibly some very accidental exceptions, "a purely self-fertilised flower." We know of no English plant which gives plainer and more easily observable evidence to the fact of self-fertilisation. This is our decided opinion after having examined more than 500 specimens of opened spathes and found in them no evidence to the contrary.

After examining these cases the reviewer will not, I think, "find it hard to tell why this book was written." But lest he should still after that find a difficulty, I will tell him myself. It was, and is, to show that artificial experiments conducted under a close-meshed net was an unnatural and very defective method to discover the operations of Nature in flowers when

exposed to the unlimited influence of sun, wind, dew and other atmospheric agencies; and to show that Nature must be interpreted under the atmospheric conditions which she herself provides, and not under those conditions minimised and in some cases almost absolutely intercepted.

AUTHOR OF "PRIMROSE AND DARWINISM."

September 2.

IN my review of "The Primrose and Darwinism," I thought it necessary to call attention to the inaccuracy of the author in the matter of quotation, but I had not the least intention of accusing him of anything more than carelessness. For instance, in the case of *Sarothamnus*, to which he refers in his letter, I was quite ready to believe that the omission of words within inverted commas was an oversight. But in his letter he tells us that they were omitted because "the bees in this reference—as they were excluded by the net—had nothing whatever to do with the subject." He stands self-convicted of knowingly altering what he quotes, but I readily believe that he is guilty of nothing worse than ignorance of the usage of literary work.

The *Field Naturalist* objects to my statement that there are "several copyist's mistakes" as well as "interpolated words" on p. 191 of his book. I therefore give the passage in his book to which I referred, followed by the corrections needed to make it agree with "Forms of Flowers," ed. ii. p. 323¹.

But in *Oxalis sensitiva* "the long-styled cleistogamic flowers are produced by long-styled plants; the mid-styled as well as the short-styled cleistogamic flowers are produced respectively by the other two forms."

The mistakes are:—

For "the long-styled read the "long-styled.

For produced by long-styled read produced by the long-styled.

For the mid-styled read and mid-styled.

For the short-styled read short-styled.

Dele, produced respectively.

If the *Field Naturalist* really considers this a justifiable sample of the art of citation I shall be surprised.

With regard to *Salvia tenori*, the *Field Naturalist* complains that I describe (p. 409) the words, "when they touched the net and the wind blew" ("The Primrose," &c., p. 11) as an incorrect quotation. When I read the phrase in question I was so much surprised to find these words attributed to Mr. Darwin that I turned to his book, where I found, "which touched the net when the wind blew." I still think that the *Field Naturalist* is not justified in placing within inverted commas a passage which does not occur in the original; nor can I agree with him that the correct and incorrect versions convey "exactly the same sense." This was the only inaccuracy in regard to *Salvia tenori* to which I called attention in my review; but I now learn, from the parallel passages given in the *Field Naturalist's* letter, that he quotes incorrectly the words "two or three flowers on the summits of three of the spikes," changing them by a not unimportant omission to "two or three flowers on the summits of the spikes."

Lastly, the *Field Naturalist* complains of my saying that he has not a "particle of evidence" for his point of view in regard to the supposed injurious effect of the net in keeping the wind from the experimental plants. He goes on: "The evidence in this case is supplied by Darwin himself. 'In all cases the flowers were protected from the wind.'" What we want is not evidence of protection from wind, but evidence that such protection has any hurtful effect on the reproductive organs of the plants.

The rest of the *Field Naturalist's* remarks do not seem to me to call for reply. THE WRITER OF THE REVIEW.

A Method of Treating Parallels.

In your issue of July 3, just to hand, Dr. Richardson suggests a method of treating parallels which differs from the orthodox Euclidean method. Improvements of a kind similar to that suggested by him will go far towards rendering the teaching of geometry more effective than it is at present. I differ from him to a slight degree in this particular instance, in that I consider it preferable to take the more general case of equal inclination of parallels to any straight line which cuts them as expressing the clearest and most useful conception of parallelism. By constituting sameness of direction the criterion of parallels—direction being purely relative, this sameness is determined by

¹ The passage is the same in edit. i.

reference to *any* other direction—the other theorems, common perpendicularity, equality of alternate angles, &c., are easily deduced.

I was pleased to read Dr. Richardson's letter, as it showed that others were working in the same direction as myself. Part of my time is devoted to teaching mathematics at the School of Mines in this town. This technical institution is attended in the evening by students who during the day are serving their apprenticeship in mechanical workshops. Although geometry is a subject which readily appeals to them, I have learnt the futility of presenting it to them under the garb of Euclid. Even if they had the courage to face the schoolboy's drilling in Euclid, I could not conscientiously ask them to devote their energies to a labour so unremunerative. I, for one, hope that Prof. Perry's efforts to harmonise the teaching of geometry and other branches of mathematics with the needs of engineering students will bear fruit, and that before the lapse of any considerable time.

W. R. JAMIESON.

Gawler, South Australia, August 27.

Symbol for Partial Differentiation.

DR. MUIR'S symbols (p. 520) may be very suitable for manuscripts or the blackboard, but the expense of printing them would be prohibitive. No book in which such symbols were used to any extent could possibly pay. On the other hand, the symbol $(dE/dv)_p$ can always be introduced into a paragraph of letterpress without using a justification or a vinculum; and this very much lessens the expense of printing.

A. B. BASSET.

Fledborough Hall, Holyport, Berks, September 26.

Bipedal Locomotion in Lizards.

I HAVE recently observed bipedal locomotion (p. 551) in the case of *Calotes versicolor* in similar circumstances to those noted by Mr. Ernest Green, and have reason to believe that it also occurs in the case of several other Agamoid lizards that I have watched in the Malay Peninsula, though their movements are too rapid to admit of certainty. *Liolepis bellii*, however, certainly uses all four legs when in rapid motion, holding its tail in the air.

N. ANNANDALE.

Lochbuie, Isle of Mull, N.B., September 25.

A Possible Meteor Shower on October 4.

ON Saturday last, October 4, at 7.45 p.m., I noticed the following phenomenon:—The sky was clouded entirely, when, happening to look to the west-north-west, I saw a well-defined streak of light, starting on a level with some trees in a small wood and moving roughly horizontally towards the south for an angular distance of about 30° . This was followed at about 3-second intervals by another and another, until I counted 43 of them. After this the interval became greater, and about 8 o'clock the phenomenon ceased. It appeared to be like a meteor shower partially hidden by a thickness of cloud. Assuming this to be true, I am afraid the radiant point was hidden by the trees before mentioned. The elevation would be about 15° . Perhaps some of your readers more favourably situated may be able to throw further light on the matter.

G. PERCY BAILEY.

Stonyhurst College, Blackburn, October 6.

FALL OF A METEORIC STONE NEAR CRUMLIN (CO. ANTRIM) SEPTEMBER 13.

THE writer of this note visited the scene of the fall of this meteorite yesterday evening, September 20, and learned that it occurred at about 10.30 a.m. (local time) on the date in question. The body is almost 10 lb. in weight and of a more or less irregular outline, and of the usual meteoric appearance. It bears strong evidence of fusion, shines with a metallic lustre on one side and is apparently truncated, a fragment—say about a third—having fractured off in its descent through the atmosphere. There is also a well-marked line or two of fracture still visible. The evidence at present is that it fell quite perpendicularly, there being no trace of slope or inclination in the hole, about 13–15 inches deep,

which it made on striking the soil. Mr. Walker, of Cross-hill, on whose holding it fell, says it was quite hot at first, and felt warm for almost an hour afterwards. Of course, a good deal of interest and local curiosity is naturally aroused, the usual query being "Where did it come from?" Possibly the data given above may help to furnish an answer to this question, although hardly yet sufficient to enable an orbit or trajectory to be computed for this—the third meteorite which has fallen in the British Isles within recent years. The occurrence was accompanied by the usual rumblings or detonations, but the estimations of the duration are here, as is usual in other similar instances, untrustworthy.

Crumlin is almost due west from Belfast, distance about 10 miles, lat. $54^\circ 36' N.$, long. $6^\circ 12' W.$

W. H. MILLIGAN.

26 Cooke Street, Belfast, September 21.

[The delay in the publication of Mr. Milligan's letter has resulted from our sending it to Mr. L. Fletcher, F.R.S., who has furnished the following interesting notes upon the meteorite.—Editor, NATURE.]

During the past fortnight it has been stated in various Irish and English newspapers that a meteoric stone had been seen to reach the earth near the village of Crumlin, a few miles distant from Belfast, on Saturday, September 13, when the meeting of the British Association in that city was in mid course.

Such reports of meteoritic falls are by no means infrequent and are almost always based on mere misapprehension of fact; indeed, it is very seldom that a stone believed to be a meteorite is found on critical examination to have any valid claim to a celestial origin. As lately as last week, for instance, a supposed meteorite was sent to the Natural History Museum from Shropshire for inspection, and yet was undoubtedly a product of our own earth.

As twenty-one years had passed away since the fall of a meteoric stone in the British Isles and thirty-seven years since the fall of a meteoric stone in Ireland, to a person in London it seemed more likely that the Crumlin fall was mythical than that a heavenly body should have fallen after so long an interval near to the very city where so many men of science were gathered together; and it seemed in any case to be a matter of certainty that before the news of the fall had reached London the stone must already have passed into the possession of a private, perhaps foreign, collector.

Last week, impressed by the circumstantial character of the reports (especially that sent by Mr. Milligan, of Belfast, for publication in NATURE), and desiring further information, I telegraphed from South Kensington to Mr. Andrew Walker, on whose farm the stone was said to have fallen; in reply he stated that the stone was still in his possession and that it had not been examined by anyone who had made a special study of meteorites. Though in doubt as to the advisability of so long a journey on the basis of such evidence as was at the moment available, I left at once for Crumlin, and was relieved on arrival to find that the journey had not been made in vain; the stone was undoubtedly a true meteorite. That a high degree of excitement had been aroused in the district by the reports of a meteoritic fall will be manifest from the circumstance that during the interview with Mr. Walker no fewer than four different sets of visitors, some in carriages, some on foot, called to see the stone and the place where it had struck the earth; each visitor was allowed to handle the specimen and feel its weight. It was being stated in the village, but Mr. Walker said it was an exaggeration, that as many as 300 people had been to the farm in the course of a single day. Although Mr. Walker had been told by some of his visitors that it would be unlucky for him to part with a gift sent to him direct from heaven, he perceived that the stone would be best preserved elsewhere